DOWNWARD NOMINAL WAGE RIGIDITY IN EUROPE

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Abstract

This paper explores the existence of downward nominal wage rigidity (DNWR) in the industry sectors of 14 European countries, over the period 1973–1999, using a data set of hourly nominal wages at industry level. Based on a novel nonparametric statistical method, which allows for country and year-specific variation in both the median and the dispersion of industry wage changes, we reject the hypothesis of no DNWR. The fraction of wage cuts prevented due to DNWR has fallen over time, from 70 percent in the 1970s to 20 percent in the 1990s, but the number of industries affected by DNWR has increased. Wage cuts are less likely in countries and years with high inflation, low unemployment, high union density and strict employment protection legislation.

JEL classification: J5, C14, C15, E31.

Keywords: downward nominal wage rigidity, European countries, employment protection legislation.

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1 Introduction

In recent years, a number of countries have adopted explicit inflation targets for monetary policy, reflecting a general agreement that monetary policy must ensure low inflation. The deliberate policy of low inflation has led to renewed interest among academics as well as policy makers for the contention of Tobin (1972) that if policy aims at too low inflation, downward rigidity of nominal wages may lead to higher wage pressure, involving higher equilibrium unemployment (see e.g. Akerlof et al., 1996, 2000, Holden, 1994, and Wyplosz, 2001). Other economists have been less concerned, questioning both the existence of downward nominal wage rigidity (DNWR), and the possible macroeconomic effects (see e.g. Gordon, 1996 and Mankiw, 1996). The issue has also received considerable attention among policy makers, cf. e.g. (ECB, 2003, OECD, 2002 and IMF, 2002).

To shed light of this issue, a fast growing body of empirical research has explored the existence of DNWR in many OECD countries (see references in section 2 below). Almost all of these studies use various kinds of micro data, mostly of the wage of individual workers, but occasionally also the wage in specific jobs in individual firms. While these studies generally seem to document the existence of DNWR, a number of key questions are still left unresolved. As the different studies vary considerably concerning both type of data and the methods that are used, it is difficult to compare the degree of DNWR across countries and the extent to which DNWR has varied over time. Furthermore, while individual data is necessary to explore whether wages are rigid at employee level, it will often be unable to answer the question of whether firms can circumvent wage rigidity at the individual level, for example by changing the composition of the workforce by turnover. Correspondingly, even if wage rigidity binds in one firm, jobs might be shifted over to other firms where wages are lower, so that the industry effects are small. Then DNWR may be less important for macroeconomic performance. It therefore seems valuable also to investigate DNWR using industry level data.

This paper explores the existence of DNWR in the industry sectors of 14 European countries, over the period 1973–1999, using a data set of hourly nominal earnings at industry level. The study is to be seen as complementary to the large number of micro studies, as it allows for comparisons across different groups of countries, and comparisons over time, based on a harmonized data set (from Eurostat). More importantly, by using data for the hourly earnings at industry level, our study captures effects of changes in the composition of the workforce, as well as the effect of changes in the wage rates. Furthermore, our study covers a number of countries in Continental Europe, for which there so far is little available evidence of the existence of DNWR, in spite of the considerable policy importance of this issue in relation to the ambitious inflation target of the ECB.

To investigate the extent of DNWR, we construct a statistical method not previously used on this issue (at least to the best of our knowledge). The advantage of the method is that it uses much weaker assumptions than most previous analyses, implying that the results should be more robust. First, the method is based on a nonparametric analysis, using data for hourly earnings only, so that no assumptions concerning explanatory variables or specific functional forms are involved. Second, we allow for country and year specific variation in the median and the dispersion of wage changes, while for instance the Kahn test (Kahn, 1997) only allows for variation in the mean.

To further explore the determinants of nominal wage ridigidy, we then regress the incidence of nominal wage cuts in each country-year sample on economic and institutional variables, like inflation, unemployment, employment protection legislation, union density, etc.

The paper is organised as follows. In Section 2, we briefly present the main theoretical explanations for DNWR, and we refer to related empirical literature. The empirical approach is laid out in Section 3, while the empirical results on DNWR are documented in Section 4. In Section 5, we explore the determinants of nominal wage rigidity. Section 6 concludes. The data we use are described in the Appendix.

2 Theoretical framework and related literature

In the literature, two alternative explanations of the existence of DNWR have been proposed. The most common explanation, advocated by e.g. Blinder and Choi (1990) and Akerlof et al. (1996), is that employers avoid nominal wage cuts because both they and (in particular) the employees think that a wage cut is unfair. The other explanation, proposed by MacLeod and Malcomson (1993) in a individual bargaining framework, and Holden (1994) in a collective agreement framework, is that nominal wages are given in contracts that can only be changed by mutual consent. For our purposes, there is no need to distinguish between these two explanations of DNWR, and, as argued by Holden (1994), they are likely to be complementary.

Concerning empirical work on DNWR, there is now a fairly large, and rapidly growing, number of recent studies, for many different countries, including Fehr and Gotte (2003) for Switzerland, Knoppik and Beissinger (2003) for Germany, Christofides and Leung (2003), and Fortin and Dumont (2000) for Canada, Holden (1998) for the manufacturing sectors in the Nordic countries, Agell and Bennmarker (2002), Agell and Lundborg (2003) and Ekberg (2002) for Sweden, Kimura and Ueda (2001) for Japan, Smith (2000), Elsby (2004) and Nickell and Quintini (2003) for the UK, and Bewley (1999), Altonji and Devereux (2000) and Lebow et al. (2003) for the US (the latter four papers also discuss previous empirical findings for the UK and the US). A preliminary multi-country study based on the European Community Household Panel is Dessy (2002). In general these studies document that nominal wages are rigid downwards. More specifically, the studies generally find (i) a spike in the distribution of nominal wage changes at zero and (ii) that the rate of inflation affects the distribution of nominal wage changes, both features indicating DNWR, With the exception of Dessy (2002), different methods and data in the above-mentioned studies make it in general difficult to compare the degree of downward nominal wage rigidity across countries.

3 Empirical approach

We use an unbalanced panel data of industry level annual wage growth from the manufacturing, mining and quarrying, electricity, gas and water supply, and construction sectors of 14 European countries in the period 1973–1999. The data source for wages are harmonized hourly earnings from Eurostat. The countries included in the sample are Belgium, Germany, Denmark, Spain, Finland, France, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Sweden and the UK. The observational unit is thus denoted Δw_{jit} where j is index for industry, i is index for country and t is index for year. There are all together $S = 5814$ observations distributed across $N = 288$ country-year samples, on average 20 industries per country-year. More details on data are provided in the appendix.

Before proceeding, let us first note that an observation of a nominal wage cut in our data differs in several respect from an observation of a nominal wage cut in most studies based on micro data. In micro studies, a nominal wage cut is usually understood as a reduction in hourly nominal pay for a job stayer. In our data, covering average hourly earnings for manual workers in an industry, a wage cut might be caused by a reduction in average hourly pay for job stayers, but it might also be caused by changes in the composition of the workers, within firms or between firms. Thus, our data involves considerable 'noise' relative to observations at the individual level, so we are unlikely to uncover all the rigidity that may exist at the individual level. On the other hand, precisely because our data also captures other 'avenues' for flexibility, it may yield a better measure of rigidity at industry level.

There are no nominal wage cuts in 210 (73%) of the country-year samples. In our data we observe, however, no less than 206 events of nominal wage reductions, i.e., 3.54% of all observations, and 21 events of zero wage change. Figure 1 shows that the number of wage cuts

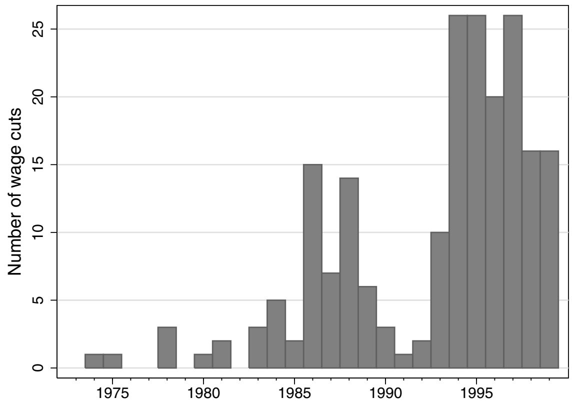


Figure 1: The number of wage cuts over time.

vary over time. There were fewer wage cuts in the 1970s and early 1980s and most wage cuts occured after 1993. The latter half of the 1980s also saw a rise in the number of wage cuts. Table A1 in the Data Appendix reports the distribution of wage cuts and observations across countries and years.

The existence of nominal wage cuts implies that DNWR is certainly not absolute, but it does not necessarily imply that wages are fully flexible; DNWR may prevent nominal wage cuts in some but not all circumstances. In Figure 2 we illustrate some country-year samples by displaying box plots of annual wage growth in Germany and the UK. We see from the figure that the average and the dispersion of wage growth vary over time between and within countries. The graphs reflect that wage cuts are rare, but it is not possible to visually detect DNWR from the graphs alone. To detect DNWR, we need to use a formal statistical method.

To this end, let us first define *the notional wage change* as the wage change that would prevail under no DNWR, following the terminology of Akerlof et al. (1996). In country-year samples where there is no effect from DNWR, the notional and observed (or empirical) wage changes will by definition be identical. However, in country-year samples where DNWR is binding, the notional and empirical wage changes will differ, as some observations of empirical wage changes will be non-negative even if the corresponding notional wage changes are negative. In such

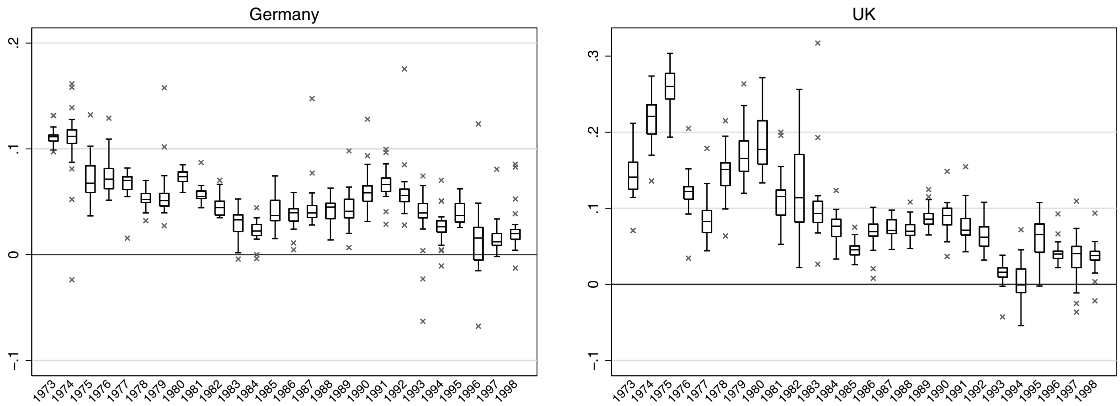


Figure 2: Box plots of annual wage growth at industry level in Germany and the UK. The box plot illustrates the distribution of wage changes within a country-year. The box extends from the 25th to the 75th percentile with the median inside the box. The whiskers emerging from the box indicate the tails of the distributions and the crosses represent outliers.

country-year samples, the distribution of empirical wage changes will be compressed from below, reducing the number of negative changes.

To detect whether the empirical distribution is compressed relative to the notional, we must obtain an estimate for the notional distribution, as well as compare the notional distributions with the empirical outcomes. We estimate the form of the notional distribution on the basis of all country-year samples, assuming the same form in all country-years, except that we allow for the median and dispersion to differ across country-year samples. This estimate based on all country-year samples may also be affected by DNWR, but less so than the distribution from one country-year sample, as the majority of the country-year samples relate to high-inflation time periods for which any DNWR is unlikely to be binding. Alternatively, we could have assumed that the notional distribution was normal, however, as illustrated in Figure 3 below, this would not be a good approximation.

To compare the notional distributions with the empirical outcomes, we simulate all country-year samples based on the notional distributions, and count the number of wage cuts in the simulations. If the empirical outcomes were affected by DNWR, the simulations based on the notional distributions will involve a higher number of wage cuts than what actually took place. If the difference between the simulated number of wage cuts, based on the notional distributions, and the actual number of wage cuts, is sufficiently large (which will be made more precise below), we conclude that DNWR has been binding in some country-year samples. In the next section, our test is presented more formally.

3.1 The formal test

To derive a distribution of notional wage changes, we exploit information from all country-year samples. As argued above, we have to make some assumptions on the form of the distribution of the nominal wage changes to be able to proceed. The box-plots in Figure 2 make clear that both the median and the dispersion of the wage changes vary over time and between countries. Thus we assume

Assumption 1 *The distribution of nominal wage changes (Δw_{jit}) is the same for all country-year samples, adjusted linearly for country-year specific median (μ'_{it}) and inter quartile range (IQR_{it}), that is $\Delta w_{jit} \sim d(\mu'_{it}, IQR_{it})$.*

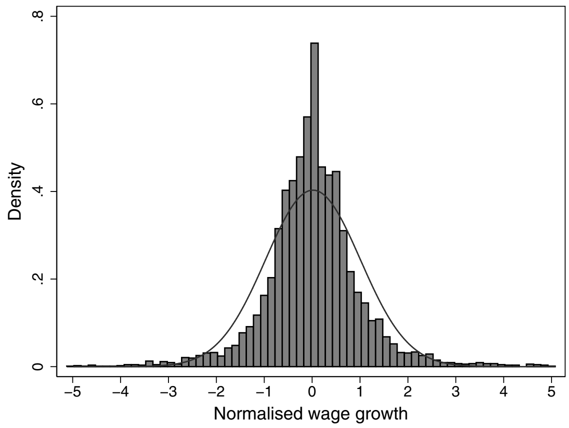
We represent the first and second order moments by the median and the inter quartile range (i.e. the distance between the 25th and 75th percentiles) because country-year samples are small, and the main alternatives, the mean and standard deviation, are less robust to outliers.[1]

Assumption 1 is not innocuous. But other tests often make stronger assumptions; for instance the Kahn test (Kahn, 1997) allows for variation over time in the mean nominal wage change, but not for variation in the dispersion. In regression based tests, significance levels are often based an assumption of normality, while no such assumption is necessary here.

Our null hypothesis is

H_0: There is no downward nominal wage rigidity.

[1]We also experimented with alternative measures like the distance between the 35th and the 75th percentiles, the median deviation form the median (MAD) and the mean deviation from the mean (MM). The correlation coefficient between the IQR and the standard deviation is 0.66, while the correlation coefficient between the other measures were always more than 0.9.



Descriptive statistics:	
Mean	0.049
Median	0
St.dev.	1.390
Skewness	10.969

Figure 3: The normalised underlying distribution of wage growth compared to a normal distribution. 38 extreme observations are omitted.

Under H_0, the empirical and the notional wage changes are identical, so that the distribution of the notional wage changes can be derived from the empirical wage changes. Employing Assumption 1 allows us to combine the observations from all country-year samples to a common underlying distribution. Specifically, we obtain a sample of 5814 observations of normalised wage changes by adjusting the empirical wage changes for the country-year specific median and inter quartile range, i.e.

$$\Delta w_s^n \equiv \left(\frac{\Delta w_{jit} - \mu_{it}^{'}}{IQR_{it}} \right), \qquad s = 1, \ldots, 5814 \tag{1}$$

For simplicity we use subscript s which runs over all j, i and t. The frequency distribution and the moments of the normalised distribution are illustrated in Figure 3. The figure compares the underlying distribution with the standard normal distribution. We notice that the underlying distribution is skewed with the mean at 5 percent.[2]

The next step is to calculate the country-year specific samples of *notional* wage changes by adjusting the normalised notional normalised wage changes, Δw_s^n, with the country-year median and inter quartile range, and compare them with the empirical country-year samples. However,

[2] We also tried country-specific normalised distributions, as well as only using observations from the high-inflation years (1973–89) (which should be less affected by DNWR), but this had little impact on the qualititative results.

as the empirical samples, as well as the moments based on them, are stochastic and thus burdened with unknown uncertainty, we use a bootstrap method. More specifically, for each of the 288 country-year samples, we bootstrap the empirical country-year sample (for example, in a country-year with 24 industries, we make 24 random draws from the empirical sample of 24 industry wage changes, with replacement). Then we

- count the number of bootstrapped wage cuts in the country-year, y_{it}^B,
- calculate country-specific bootstrapped median, $\mu_{it}^{'B}$, and inter quartile range, IQR_{it}^B,
- construct the country-year specific distribution of notional wage changes as

$$\Delta\widetilde{w}_s^{it} \equiv \left(\Delta w_s^n IQR_{it}^B + \mu_{it}^{'B}\right), \qquad s = 1, \ldots, 5814 \tag{2}$$

- calculate the corresponding country-year specific probabilities of a notional wage cut in country-year it as the proportion of wage cuts out of the total sample of observations S

$$\tilde{q}_{it} \equiv \frac{\#\Delta\tilde{w}_s^{it} < 0}{S}, \qquad s = 1, \ldots, 5814 \tag{3}$$

- simulate the number of wage cuts in each country-year specific notional sample, $\widehat{y}_{it}$, by drawing from a binomial distribution using the country-specific notional probabilities $\tilde{q}_{it}$, and
- compare the total number of bootstrapped wage cuts $Y^B = \sum_{it} y_{it}$ for all 288 country-year samples with the total number of simulated notional wage cuts, $\widehat{Y} = \sum_{it} \widehat{y}_{it}$.

If the empirical samples are affected by DNWR, there will be a tendency that there are more simulated wage cuts than bootstrapped wage cuts, i.e. $\widehat{Y} > Y^B$. We repeat this procedure 5000 times, undertaking a new bootstrap for each country-year sample each time, and count the

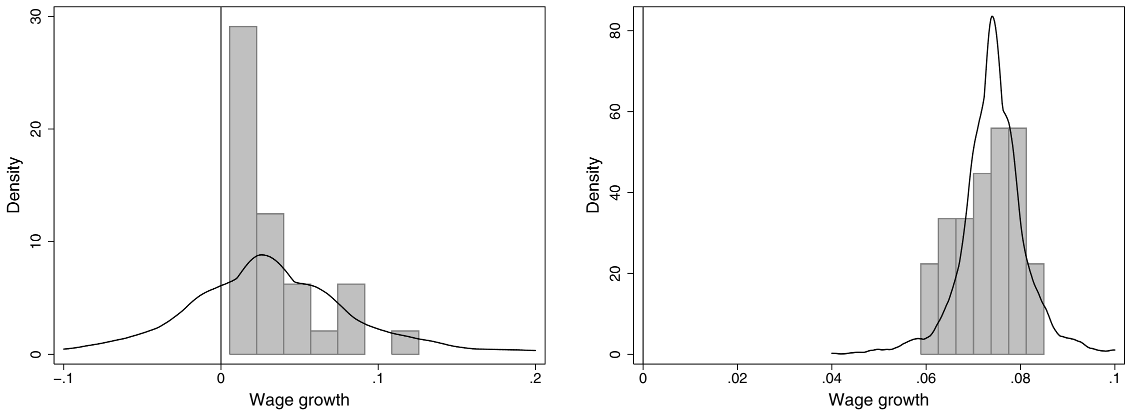


Figure 4: Empirical (histograms) and notional (lines) distributions for Portugal (1997) and Germany (1980)

number of times where $\widehat{Y} > Y^B$. The null hypothesis is rejected with a level of significance at 5% if $1 - \#(\widehat{Y} > Y^B)/5000 \leq 0.05$.

Before we turn to the results, let us make two brief comments as to the method. As noted above, if DNWR is at work in some country-year samples, the empirical wage distribution will be compressed, and so will our estimate of the notional wage changes, as it is based on the empirical distributions for all country-year samples. Thus, notional probabilities will also be biased downwards, reducing the number of simulated wage cuts, which will reduce the power of our test. Under H_0, however, there is no DNWR, and therefore no compression. Thus, under H_0, the notional probabilities are unbiased, so this feature will not affect the significance level of our test.

Samples of the empirical and notional wage changes for two country-years are compared in Figure 4 (these figures are based on the empirical country-year samples without bootstrap). The empirical histograms represents typically only 20 observations while there are 5814 observations behind the notional ones (for the latter, we use kernel densities based on the Epanechnikov function). We see in the left diagram that the notional distribution have a positive mass below zero while there were no observed wage cuts, consistent with the hypothesis that downward nominal wage rigidity prevents wage cuts and thus compresses the distribution of the empirical

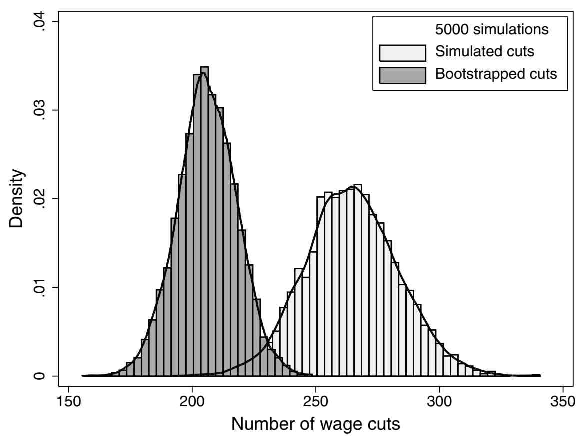


Figure 5: The frequency distributions of the number of 5000 bootstrapped (empirical) and simulated (notional) wage cuts.

samples. The right panel illustrate a country-year where the distributions of both observed and notional wage growth are entirely above zero.

4 Results

In 4995 of 5000 simulations, there are more simulated than bootstrapped wage cuts. Thus we reject the null hypothesis comfortably with a p-value of 0.001, and we may conclude that DNWR has been at work in our sample. To illustrate the power of the test we plot the histogram of the number of simulated and bootstrapped wage cuts in Figure 5. On average, we simulate 265 notional wage cuts and bootstrap 206 wage cuts (due to the large number of simulations, the bootstrapped average of 206 clearly equals the number of observed wage cuts). The average fraction of notional wage cuts that do not result in an observed wage cut due to DNWR, may be expressed by $(1 - Y/\widehat{Y})$ where Y is the number of observed wage cuts and $\widehat{Y}$ is the average number of simulated cuts. For the whole sample this fraction is $(1 - 206/265) = 0.22$. Thus, a bit more than one out of five notional wage cuts does not result in an observed wage cut due to DNWR. Another measure which illustrates the economic significance of DNWR is the average fraction of industries affected by DNWR. This fraction may be calculated by $(\widehat{Y} - Y)/S$ where S

is the number of industries. For the whole sample this fraction is $(265 - 206/5814) = 0.010$.

Given the evidence of DNWR, it is clear that our method of estimating the notional wage changes based on all wage observations, including those affected by DNWR, implies that also the notional wage distribution is affected by DNWR. This causes the notional probabilities to be biased downwards, implying that the numbers above for the wage cuts prevented and for industries affected by DNWR, will also be biased downwards. The bias is reflected in the skewness present in the underlying distribution of wage growth shown in Figure 3. To circumvent this bias, we in the sequel follow Card and Hyslop (1997) by assuming that the distribution of wage changes is symmetric in the absence of DNWR (in Appendix B, we include results without the symmetry assumption). Specifically, we assume that DNWR affects the lower half of the notional distribution while the upper half is unaffected. As pointed out by Card and Hyslop (1997), most conventional models of wage determination imply symmetry. Furthermore, it can be shown that if the distribution of relative wages is stationary, then the distribution of changes is symmetric. On the other hand, Elsby (2004) shows that DNWR is likely to affect also the upper tail of the distribution, as wage setters may set lower wage increases in years where DNWR does not bind, to reduce the risk that DNWR will bind in the future. However, as the upper tail is less affected by DNWR than the lower tail, and in any case the normalised distribution is based on many years for which inflation is too high for DNWR to be an important problem, we don't think this is critical.

To obtain a normalised and symmetric distribution of notional wage changes, we choose observations greater than median of the underlying distribution in Figure 3 and flip them around the median. From this distribution we obtain bias adjusted notional wage growth distributions and corresponding bias adjusted notional probabilities. Otherwise, the method is as described above. As expected, the average number of simulated wage cuts (295) are somewhat higher using the bias-adjusted probabilities, implying that the fraction of wage cuts prevented for the entire sample increases from 0.22 to 0.30, cf. column 1 in Table 2.

Table 1: Results from 5000 simulations on subperiods using bias adjusted probabilities.

Sample properties:	1973–1979	1980–1989	1990–1994	1995–1999
No. of observations	1326	2180	1200	1108
No. of country-years	67	113	56	52
Average wage growth	14.99%	9.27%	6.68%	4.38%
Average inflation rate	10.68%	8.69%	4.99%	2.37%
Average unemployment rate	3.80%	8.71%	9.03%	9.21%
Observed wage cuts (Y)	5	55	42	104
Proportion of wage cuts (%)	0.38	2.52	3.50	9.39
Simulation results:				
Average simulated wage cuts	17	84	62	131
$\#(\widehat{Y} > Y^B)$	4985	4983	4939	4929
Probability of significance	0.003	0.003	0.012	0.014
Fraction of wage cuts prevented	0.706	0.345	0.323	0.206
Fraction of industries affected	0.009	0.013	0.017	0.024

A number of interesting questions arise. Is there evidence for DNWR for different time periods, regions and countries? To what extent is DNWR related to labour market institutions as proposed by theory? We first investigate whether DNWR has changed over time by splitting the sample into four subperiods 1973–1979, 1980–1989, 1990–1994 and 1995–1999, see Table 1.

There is evidence of DNWR in all periods. In the high-inflation 1970s, on average 70 percent of the notional wage cuts did not result in observed wage cuts. In the 1980s and early 1990s, more than 30 percent of the notional wage cuts did not result in observed wage cuts, while in the late 1990s, the probability that DNWR prevented a notional wage cut leading to an observed wage cut was 20 percent. While the results indicate that the fraction of wage cuts prevented by DNWR decreased over time, the average fraction of industries affected by DNWR increased from 0.9 percent in the 1970s to 1.3 percent in the 1980s, 1.7 percent in the early 1990s and finally 2.4 percent in the late 1990s.

Nominal rigidities may also be related to labour market institutions. Based on a theoretical framework allowing for bargaining over collective agreements as well as individual bargaining, Holden (2004) argues that workers who have their wage set via unions or collective agreements

Table 2: Results from 5000 simulations on regions using bias adjusted probabilities.

Sample properties:	All regions	British Isles and Denmark	Core	South
No. of observations	5814	1565	2697	1473
No. of country-years	288	74	132	75
Observed wage cuts (Y)	206	53	122	29
Proportion of wage cuts (%)	3.54	3.39	4.52	1.97
Simulation results:				
Average simulated wage cuts	295	76	160	55
$\#(\widehat{Y} > Y^B)$	5000	4934	4993	4994
Probability of significance	0	0.013	0.001	0.001
Fraction of wage cuts prevented	0.302	0.303	0.238	0.473
Fraction of industries affected	0.015	0.015	0.014	0.018

have stronger protection against a nominal wage cut, thus the extent of DNWR is likely to depend on the coverage of collective agreements and union density. For non-union workers, the strictness of the employment protection legislation (EPL) is key to their possibility of avoiding a nominal wage cut. As documented by among others OECD (1999), such institutions differ considerably among European countries, and it would therefore be interesting to investigate existence of DNWR for regions as well as individual countries. We first split the sample into regions which have comparable labour market institutions. We operate with three regions; the British Isles and Denmark, Core (Belgium, France, Germany, Luxembourg and the Netherlands), and South (Italy, Greece, Portugal and Spain).[3] The results from simulations using these regions are presented in columns 2, 3 and 4 in Table 2.

We reject the hypothesis of no DNWR for all regions. In the South, a region where bargaining coverage is fairly high (see e.g. Calmfors et al., 2001, table 4.4) and EPL is very strict OECD (1999), 47 percent of the notional wage cuts did not result in an observed cut, while 1.8 percent of the industries were affected by DNWR. In the Core, where there is generally high bargaining coverage and fairly strong EPL, 24 of the notional wage cuts did not result in observed cuts, which is considerably lower than in the South. In the British Isles and Denmark, 30 percent

[3]We omit Sweden and Finland because they differ from the other countries in this respect and because of too few observations.

of the notitional wage cuts were prevented by DNWR. In this region, EPL is less strict than in most of the rest of Europe; however, union density and bargaining coverage are fairly high in Denmark and Ireland, but not in the UK. 1.4 percent of the industries were affected by DNWR in the Core, and 1.5 percent the British Isles and Denmark.

Splitting the sample by combining the regions and the sub-periods reduces the significance levels, see Table 3. At ten percent level, we find significant DNWR in the British Isles and Denmark (1980s), the Core (1970s and 1980s), and in the South (all periods). For all regions, the fraction of notional wage cuts that did not lead to observed cuts has fallen over time, consistent with the aggregate picture as seen in Table 1. The fraction of industries affected by DNWR has increased in all regions, the exception being the British Isles and Denmark in the late 1990s.

In Table 4, we report the results concerning individual countries. These results should be treated more cautiously, as they are based on a smaller number of observations. Bearing this in mind, we observe that for all countries except France, the simulations indicate that some of the notional wage cuts do not result in observed wage cuts due to DNWR. For four countries (Belgium, Italy, the Netherlands, and Portugal), DNWR is significant at the five percent level, while for Denmark, Ireland and Luxembourg, we find DNWR at ten percent level. For the other countries, DNWR is not statistically significant, even if the average fraction of notional wage cuts that do not result in observed cuts for some countries is as high as 16 percent for Germany and 25 percent for the UK. This illustrates the considerable uncertainty involved in this measure. The fraction of industries affected by DNWR varies from 5.8 (the Netherlands) and 4.5 (Portugal) percent at the top, to 0.4 (Greece) and 0 (France) percent at the bottom.

To further explore the reliability of our measures of DNWR, we undertake Poisson regressions with the number of observed wage cuts in each country-year sample it, Y_{it}, as the dependent variable, and normalise on the number of simulated wage cuts, $\widehat{Y}_{it}$. A Poisson regression seems appropriate as the endogenous variable is based on count data, see Cameron and Trivedi (1998).

Table 3: Results from 5000 simulations on regions and sub-periods using bias adjusted probabilities.

Region		1973–1979	1980–1989	1990–1994	1995–1999
	No. of observations	369	644	313	239
	No. of country-years	18	30	15	11
British	Observed wage cuts (Y)	1	11	20	21
Isles	Proportion of wage cuts (%)	0.27	1.71	6.39	8.79
and	Average simulated wage cuts	4	20	27	25
Denmark	$\#(\widehat{Y} > Y^B)$	4330	4711	4347	3627
	Probability of significance	0.134	0.058	0.131	0.275
	Fraction of wage cuts prevented	0.750	0.450	0.259	0.160
	Fraction of industries affected	0.008	0.014	0.022	0.017
	No. of observations	698	1023	506	470
	No. of country-years	35	50	25	22
	Observed wage cuts (Y)	4	39	17	62
Core	Proportion of wage cuts (%)	0.57	3.81	3.36	13.19
	Average simulated wage cuts	11	54	24	71
	$\#(\widehat{Y} > Y^B)$	4760	4777	4398	4344
	Probability of significance	0.048	0.045	0.120	0.131
	Fraction of wage cuts prevented	0.636	0.278	0.292	0.127
	Fraction of industries affected	0.010	0.015	0.014	0.019
	No. of observations	259	513	366	335
	No. of country-years	14	33	15	13
	Observed wage cuts (Y)	0	5	4	20
South	Proportion of wage cuts (%)	0	0.97	1.09	5.97
	Average simulated wage cuts	3	11	11	31
	$\#(\widehat{Y} > Y^B)$	4613	4629	4780	4740
	Probability of significance	0.077	0.074	0.044	0.052
	Fraction of wage cuts prevented	1	0.545	0.636	0.355
	Fraction of industries affected	0.012	0.012	0.019	0.054

Adding dummys for region, period, combined region and period, as well as for countries, we are then able to derive confidence intervals for the fraction of wage cuts prevented for all the respective subsamples, see Figure 6. Note that the point estimates of the fractions in Figure 6 differ slightly from the fractions in the tables, as the former are based on the Poisson regressions, and thus are non-linear, while the latter are linear averages based on the simulations. The confidence intervals are fairly large, and with few exceptions, we are not able to conclude that the fractions are significantly different from one another.

However, we also undertake a Poisson regression of Y_{it}, as the dependent variable, normal-

Table 4: Results from 5000 simulations on countries using bias adjusted probabilities.

Country	No. of observations	No. of years	Observed wage cuts (Y)	Prop. of wage cuts (%)	Average simulated wage cuts	$\#(\widehat{Y} > Y^B)$	Probability of significance	Fraction of wage cuts prevented	Fraction of industries affected
Belgium	575	26	31	5.39	42	4787	0.043	0.262	0.019
Denmark	485	25	8	1.65	14	4507	0.099	0.429	0.012
France	554	26	21	3.79	21	2329	0.534	0	0
Germany	665	26	16	2.41	19	3331	0.334	0.158	0.005
Greece	472	26	7	1.48	9	3099	0.380	0.222	0.004
Ireland	462	23	27	5.84	38	4632	0.074	0.289	0.024
Italy	312	13	0	0	4	4864	0.027	1	0.013
Luxembourg	420	27	31	7.38	41	4617	0.077	0.244	0.024
Netherlands	431	27	12	2.78	37	4918	0.016	0.676	0.058
Portugal	401	18	2	0.50	20	4999	0.000	0.900	0.045
Spain	288	18	20	6.94	22	3091	0.382	0.091	0.007
UK	615	26	18	2.93	24	4184	0.163	0.250	0.010

ising on $\widehat{Y}_{it}$, and adding a time trend. The estimated trend coefficient is -0.035 and is significantly negative at the one percent level, implying that we can conclude that DNWR as measured by the fraction of wage cuts prevented, has fallen over time. Furthermore, we also regress the country-year observations of the fraction of industries affected, $(\widehat{Y}_{it} - Y_{it})/S_{it}$ on a time trend (now using OLS, as a Poisson regression is not feasible when some observations are negative). We find a trend coefficient of 0.00066, which is significantly positive at the one percent level, implying that the number of industries affected by DNWR has increased over time.

5 Explaining the number of wage cuts

While the previous analysis documents the existence of DNWR, it does not shed light on to what extent the incidence of nominal wage cuts depends on economic and institutional variables. Treating the number of wage cuts in each country-year sample as one observation, we have 288 observations. As mentioned above, Holden (2004) shows that the incidence of wage cuts is likely

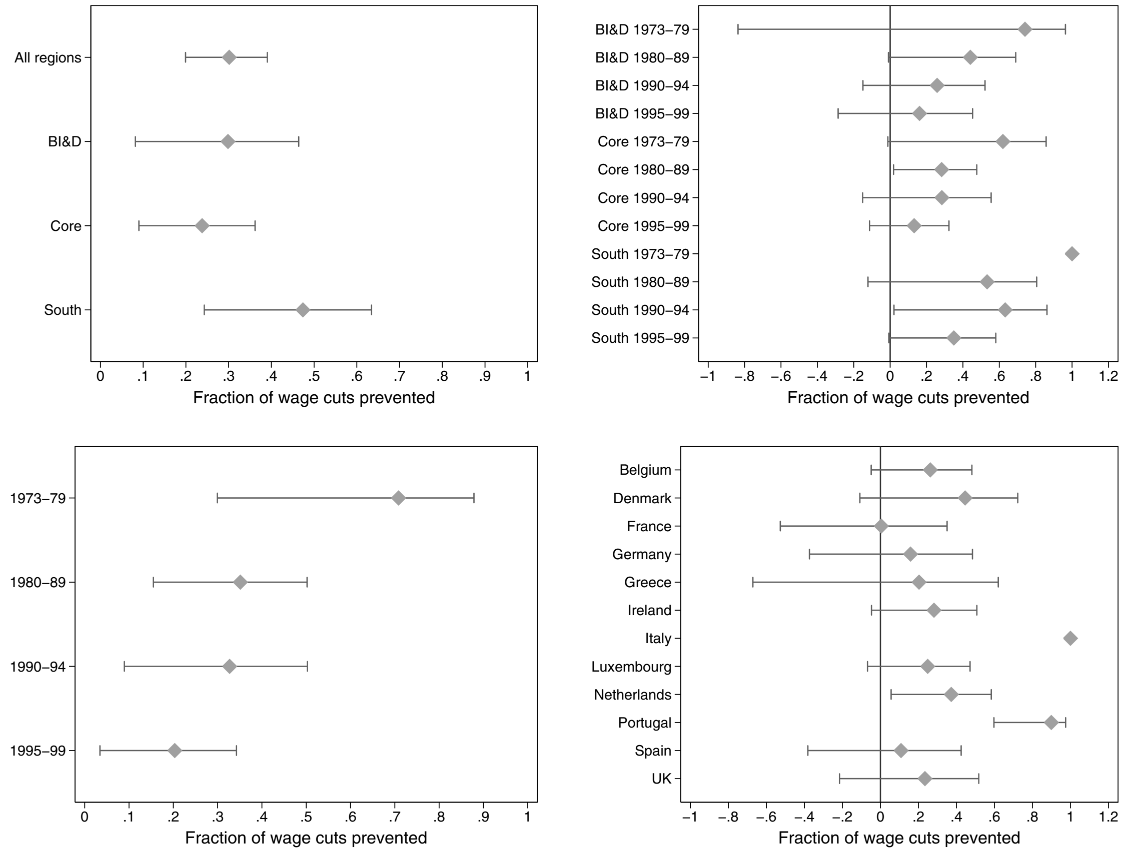


Figure 6: Estimated fractions of wage cuts prevented with 95% confidence intervals.

to depend on inflation in a non-linear way, as well as on institutional variables like EPL and union density/bargaining coverage. Furthermore, high unemployment may also weaken workers' resistance to nominal wage cuts. Thus, we apply a Poisson regression model of the number of wage cuts in each country-year sample, with a number of explanatory variables including inflation and inflation squared, an index of EPL, union density, the unemployment rate, as well as an interaction between EPL and inflation.[4] We do the analysis in two different ways. First, we normalise on the number of industries in the country-year sample, S_{it}, i.e. we explain the incidence of wage cuts. Second, we normalise on the number of simulated wage cuts, $\widehat{Y}_{it}$, i.e. we explain the fraction of wage cuts prevented. Adding institutional variables as regressors, we can then test

[4]Regrettably, the data for union density and bargaining coverage apply to the whole economy, and not to the industry sector. As variation in density or coverage in other parts of the economy would affect the density/coverage variable, but presumably not affect wage setting in the industry sector, the estimates of these variables might be biased downwards. See further details in the Data Appendix.

directly whether these variables lead to fewer observed that notional wage cuts, i.e. to DNWR.

The conditional density in a Poisson model is

$$f(Y_{it} = y_{it} \mid \mathbf{x}_{it}) = \frac{e^{-\lambda_{it}}\lambda_{it}^{y_{it}}}{y_{it}!} \tag{4}$$

where $E(Y_{it} \mid \mathbf{x}_{it}) = \lambda_{it}$, and

$$\ln\lambda_{it} = \mathbf{x}_{it}'\boldsymbol{\beta} \tag{5}$$

where $\mathbf{x}_{it}$ represents the explanatory variables and $\boldsymbol{\beta}$ is the parameter vector. In the Poisson model the variance is equal to the mean. However, data are often characterised by 'overdispersion' and hence at odds with the Poisson assumption. Undertaking the Poisson regression of Y_{it}/S_{it}, a goodness-of fit test formally rejects the hypothesis that the data are generated according to the Poisson regression model ($\chi^2(254) = 389.6$). We therefore use a negative binomial regression model, which allows for overdispersion and can be seen as a generalisation of the Poisson model. Specifically, we use two alternative specifications for the Poisson parameter:

$$\ln\lambda_{it} = \mathbf{x}_{it}'\boldsymbol{\beta} + \varepsilon_{it}, \quad \varepsilon_{it} \sim \Gamma(1, \delta) \tag{5'}$$

$$\ln\lambda_{it} = \mathbf{x}_{it}'\boldsymbol{\beta} + \varepsilon_{it}, \quad \varepsilon_{it} \sim \Gamma(1, \phi_i e^{-\alpha_i}) \tag{5''}$$

Including a Gamma distributed error term, ε_{it}, in (5') and (5") allows the variance to mean ratios of Y_{it} to be larger than unity. (4) and (5') yield the pooled negative binomial regression model. In (5"), we also include a country specific fixed effect, α_i, to allow for a country specific variance to mean ratio, see Hausman et al. (1984) for details.

The results of the negative binomial model are presented in the first two columns of Table 5. In accordance with the theoretical predictions, EPL, union density and inflation, all have a significant negative effect on the incidence of nominal wage cuts, although in the fixed effects model, union density is only significant at the 10 percent level. High unemployment reduces the

Table 5: Maximum likelihood estimates with standard errors in parenthesis.

	Negative binomial		Poisson	
	Pooled	Fixed effects	Pooled	Fixed effects
Ln(S_{it})	1 (–)	1 (–)	–	–
Ln(Simulated cuts)	–	–	1 (–)	1 (–)
EPL	−0.745* (0.277)	−0.969* (0.367)	−0.307* (0.133)	−0.767* (0.335)
Union density	−1.739* (0.795)	−2.210 (1.299)	−1.072* (0.514)	−5.843 (3.118)
Inflation	−0.773* (0.168)	−0.609* (0.157)	−0.246* (0.097)	−0.250 (0.132)
Inflation squared	0.012* (0.005)	0.004 (0.005)	0.003 (0.003)	−0.089 (0.424)
Unemployment	0.074* (0.032)	0.104* (0.043)	0.028 (0.017)	0.039 (0.044)
EPL × inflation	0.111 (0.057)	0.130* (0.062)	0.041 (0.032)	7.582 (4.615)
constant	−0.076 (0.927)	−1.041 (1.319)	0.827 (0.471)	–
log-likelihood	–218.9	–182.7	–151.3	–116.9
Number of observations	261	246	261	246

Notes: (i) S_{it} is the number of industries in country-year sample it. (ii) * indicates significance at 5% level. (iii) Luxembourg is not included because of lack of EPL data. In addition, Finland and Italy are excluded from the fixed effects models as there are no observed wage cuts in these countries.

incidence of wage cuts. The interaction between EPL and inflation is significantly positive in the fixed effects model, reflecting that the negative effect of EPL on the incidence of wage cuts do not apply in high-inflation periods, when wage cuts are rare irrespective of EPL. We also tried to include bargaining coverage, an index of temporary employment and the interaction of inflation and union density in the fixed effects model. They all entered with the expected sign, but they were jointly insignificant with a $\chi^2(3) = 2.37$.

We then investigate whether institutions affect the extent of DNWR as measured by the average fraction of wage cuts prevented $(1 - Y/\widehat{Y})$, by a Poisson regression of Y_{it} normalised on the number of simulated wage cuts $\widehat{Y}_{it}$. The results are presented in columns 3 (pooled) and 4 (fixed effects) of Table 5. Note that in this case the restriction imposed by the Poisson regression relative to the negative binomial regression is accepted easily; indeed the results are the same in the negative binomial model for both specifications.[5] Again, we find a significant negative effect on the number of wage cuts, implying a positive effect of EPL and union density on the fraction of wage cuts prevented. Also, there is a positive effect of inflation, which implies that there are

[5]The goodness-of-fit test yields $\chi^2(254) = 133.0$.

fewer wage cuts prevented when inflation is lower. In the fixed effects model, union density and inflation are only significant at the ten percent level. We also tried to include bargaining coverage, an index of temporary employment and the interaction of inflation and union density in the fixed effects model. They all entered with the expected sign, but they were jointly insignificant with a $\chi^2(3) = 1.37$.

6 Conclusions

This paper explores the existence of downward nominal wage rigidity (DNWR) in the manufacturing, mining and quarrying, electricity, gas and water supply, and construction sectors of 14 European countries, over the period 1973–1999, using a data set of hourly nominal wages at industry level. Based on a novel nonparametric statistical method, which allows for country and year specific variation in both the median and the dispersion of industry wage changes, we reject the hypothesis of no DNWR for the total sample. Splitting into subsamples, we document the existence of DNWR for the high inflation period 1973–1989, as well as for the low inflation periods 1990–1994 and 1995–1999. Furthermore, we also find evidence for DNWR for groups of countries: the South (Italy, Greece, Portugal, Spain), the Core (Belgium, France, Germany, Luxembourg, Netherlands), and the British Isles and Denmark. Dividing further into individual countries, the results indicate that, for all countries except France, some of the notional wage cuts do not lead to observered wage cuts due to DNWR. However, DNWR is statistically significant only for some of the countries: for Belgium, Italy, the Netherlands, and Portugal at five percent level, and Denmark, Ireland and Luxembourg at ten percent level.

Interestingly, our results show that the fraction of notional wage cuts that do not result in observed wage cuts has fallen over time, for all the groups of countries we consider. The simulations indicate that for all countries together, the fraction of wage cuts prevented has fallen from 70 percent in the 1970s to 20 percent in the late 1990s. On the other hand, as the inflation has fallen over time, the fraction of industries affected by DNWR has increased from less than 0.9 percent in the 1970s, to 2.4 percent in the late 1990s.

We then proceed to explore whether the incidence of nominal wage cuts can be explained by economic and institutional variables. Treating the incidence of nominal wage cuts in each country-year sample as one observation, we find significant negative effect of inflation, the strictness of employment protection legislation and of union density. We also find that inflation, the strictness of employment protection legislation and union density have significant positive impact on our measure of DNWR: in country-year samples with high inflation, strict employment protection legislation and high union density, the number of observed wage cuts is significantly reduced relative to the number of simulated, notional wage cuts.

Our study should be seen as complementary to the increasing number of empirical studies on the existence of DNWR based in individual data. Compared to these studies, our approach has the advantage that it focusses on industry level effects, and thus is not subject to the critique that significant DNWR at individual or firm level might be circumvented by employment being shifted over from high-wage to low-wage jobs. In comparison, Card and Hyslop (1997) find evidence of DNWR on US microdata, but inconclusive evidence for state level data. On the other hand, as we (obviously) have much fewer observations than most micro-studies, and use weak assumptions – no functional form assumption, and allowing for time and country variation in the median and dispersion of wage changes – our test presumably has lower power. Indeed, Knoppik and Beissinger (2003) find significant DNWR for Germany, while we do not.

We are reluctant to draw strong policy conclusions from our study. Overall in our sample, DNWR is significant but of moderate size. Labour markets appear to adapt to lower inflation, as the fraction of wage cuts prevented by DNWR has fallen over time. Yet the fraction of total industries that have been affected by DNWR has increased over time, suggesting that the overall effect on DNWR of a more determined effort towards low inflation, as the monetary policy of the

ECB arguably implies, are uncertain.

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A Data appendix

We have obtained our wage data from Eurostat. The precise source is Table HMWHOUR in the *Harmonized earnings* domain of under the *Population and Social Conditions* theme in the NEWCRONOS database. Our wage variable (HMWHOUR) is labelled *Gross hourly earnings of manual workers in industry*. Gross earnings cover remuneration in cash paid directly and regularly by the employer at the time of each wage payment, before tax deductions and social security contributions payable by wage earners and retained by the employer. Payments for leave, public holidays, and other paid individual absences, are included in principle, in so far as the corresponding days or hours are also taken into account to calculate earnings per unit of time. The weekly hours of work are those in a normal week's work (i.e. not including public holidays) during the reference period. These hours are calculated on the basis of the number of hours paid, including overtime hours paid. Furthermore, we use data in national currency and males and females are both included in the data. The data for Germany does not include GDR before 1990 or new *Länder*.

The data are recorded by classification of economic activities (NACE Rev. 1). The sections represented are Mining and quarrying (C), Manufacturing (D), Electricity, gas and water supply (E) and Construction (F). We use data on various levels of aggregation from the section levels (e.g. D Manufacturing) to group levels (e.g. DA 159 Manufacturing of beverages), however, using the most disaggregate level available in order to maximize the number of observations. If for example, wage data are available for D, DA 158 and DA 159, we use the latter two only to avoid counting the same observations twice.

The average number of observations per country-year sample is 20.5, with a standard error of 4.7. The distribution of the number of wage cuts relative to the number of observations on years and countries are reported in Table A1.

Data for inflation and unemployment are from the OECD Economic Outlook database.

The primary sources for the employment protection legislation index, which is displayed in Table A2, are Lazear (1990) for the period 1973–79 and OECD (1999) for the remaining years. We follow the same procedure as Blanchard and Wolfers (2000) to construct time-varying series which is to use the OECD summary measure in the 'Late 1980s' for 1980–89 and the 'Late 1990s' for 1995–99. For 1990-94 we interpolate the series, and use the percentage change in Lazear's index to back-cast the OECD measure. However, we are not able to reconstruct the Blanchard and Wolfers data exactly.

Data for union density until 1995 is from Nickell et al. (2002, Table 5). For the remaining years we interpolate using observations for 2001 from EIRO (2003, Table 9). Data for Greece (1985 and 1995), Ireland (1985 and 1993) and Luxembourg (1987 and 1995) are from ILO (1997, Table 1.2). Data for intervening years are produced by interpolation, while we extrapolate before 1985(87) and after 1993(95).

Data for bargaining coverage until 1994 are from Nickell et al. (2002, Table 4), which provide data with five year intervals. Yearly data are calculated by interpolation. EIRO (2003, Table 1) presents data for 2000 (1999 for Portugal and 2001 for the Netherlands) which allows us to interpolate for the late 1990s. Data for Greece and Ireland are only available for 1994 from ILO (1997, Table 1.2).

Data for the incidence of temporary employment is from Young (2003, Table 4.1), which provides observations with five year intervals from 1985–2000. We interpolate to obtain yearly data and extrapolate before 1985.

Table A1: The distribution of nominal wage cuts relative to the number of observations by countries and years

Year	*Belgium* *BE*	*Germany* *DE*	*Denmark* *DK*	*Spain* *ES*	*Finland* *FI*	*France* *FR*	*Greece* *GR*	*Ireland* *IE*	*Italy* *IT*	*Luxembourg* *LU*	*Netherlands* *NL*	*Portugal* *PT*	*Sweden* *SW*	*UK* *UK*	*Total*
1973	0/20	0/23	0/20	–	–	0/20	0/13	–	0/24	0/14	0/19	–	–	0/21	0/174
1974	0/20	1/23	0/20	–	–	0/21	0/13	–	0/24	0/14	0/19	–	–	0/21	1/175
1975	0/20	0/24	1/20	–	–	0/22	0/13	–	0/24	0/15	0/19	–	–	0/21	1/178
1976	0/21	0/24	0/20	–	–	0/22	0/13	0/18	0/24	0/15	0/19	–	–	0/23	0/199
1977	0/21	0/24	0/20	–	–	0/22	0/13	0/18	0/24	0/15	0/19	–	–	0/23	0/199
1978	0/21	0/24	0/20	–	–	0/22	0/13	0/18	0/24	2/15	0/20	–	–	0/23	3/200
1979	0/21	0/24	0/21	–	–	0/22	0/13	0/20	0/24	0/15	0/19	–	–	0/22	0/201
1980	0/21	0/24	1/21	–	–	0/22	0/13	0/20	0/24	0/15	0/19	–	–	0/22	1/201
1981	0/21	0/24	0/21	–	–	0/22	0/13	0/20	0/24	2/15	0/19	0/22	–	0/22	2/223
1982	0/21	0/24	0/21	0/2	–	0/21	0/13	0/20	0/24	0/16	0/18	0/22	–	0/22	0/224
1983	0/21	1/24	0/21	1/2	–	0/21	0/11	0/18	0/24	0/16	0/18	0/22	–	0/24	3/222
1984	0/21	1/27	0/21	0/2	–	0/22	0/17	0/18	0/24	1/16	0/16	0/22	–	0/24	5/230
1985	0/21	0/27	0/21	0/2	–	0/23	0/18	1/20	0/24	1/16	0/17	0/22	–	0/24	2/235
1986	6/21	0/27	2/21	0/2	–	2/23	2/18	1/21	–	0/14	0/18	0/22	–	0/24	15/211
1987	0/21	0/27	0/21	0/2	–	1/23	0/18	3/20	–	3/14	0/18	0/22	–	0/24	7/210
1988	3/21	0/27	0/21	0/2	–	5/23	0/18	1/20	–	3/14	0/18	0/20	–	0/25	14/211
1989	0/22	0/27	0/21	0/2	–	1/23	0/18	2/20	–	0/17	0/17	2/23	–	0/26	6/213
1990	0/24	0/27	0/21	0/26	–	1/23	0/25	1/21	–	1/16	0/17	0/22	–	0/25	3/248
1991	0/24	0/27	0/21	0/26	–	1/23	0/25	0/21	–	0/17	0/17	0/22	–	0/25	1/248
1992	0/23	0/24	1/21	0/26	–	0/23	1/25	0/21	–	0/17	0/17	0/22	–	0/25	2/244
1993	0/22	2/24	2/21	1/26	–	2/23	0/25	1/21	–	0/17	0/14	0/22	–	2/25	10/240
1994	0/22	1/26	0/2	2/26	–	8/14	0/25	2/21	–	1/17	0/8	0/22	1/15	11/22	26/220
1995	19/22	0/26	–	0/26	–	0/9	0/25	6/20	–	0/17	0/10	0/22	0/15	1/21	26/213
1996	0/27	7/25	–	4/26	–	0/10	0/25	2/23	–	6/18	0/20	0/22	0/15	0/26	20/237
1997	2/28	2/31	0/16	7/30	0/2	0/28	1/25	4/23	–	6/13	1/23	0/22	0/15	3/27	26/283
1998	0/28	1/31	0/16	3/30	0/2	0/27	3/24	3/23	–	4/16	0/23	0/28	1/15	1/28	16/291
1999	–	–	1/16	2/30	–	–	–	–	–	1/16	12/22	–	–	–	16/84
Total	31/575	16/665	8/485	20/288	0/4	21/554	7/472	27/462	0/312	31/420	23/431	2/401	2/75	18/615	206/5814

Table A2: Indices for employment protection legislation

Year	*BE*	*DE*	*DK*	*ES*	*FI*	*FR*	*GR*	*IE*	*IT*	*NL*	*PT*	*SW*	*UK*
1973	3.10	3.20	2.10	3.89	2.30	2.44	3.60	0.76	4.10	2.70	3.16	2.57	0.46
1974	3.10	3.20	2.10	3.89	2.30	2.57	3.60	0.83	4.10	2.70	3.42	3.03	0.48
1975	3.10	3.20	2.10	3.89	2.30	2.70	3.60	0.90	4.10	2.70	3.67	3.50	0.50
1976	3.10	3.20	2.10	3.86	2.30	2.70	3.60	0.90	4.10	2.70	3.75	3.50	0.50
1977	3.10	3.20	2.10	3.82	2.30	2.70	3.60	0.90	4.10	2.70	3.83	3.50	0.50
1978	3.10	3.20	2.10	3.78	2.30	2.70	3.60	0.90	4.10	2.70	3.92	3.50	0.50
1979	3.10	3.20	2.10	3.74	2.30	2.70	3.60	0.90	4.10	2.70	4.00	3.50	0.50
1980	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1981	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1982	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1983	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1984	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1985	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1986	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1987	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1988	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1989	3.10	3.20	2.10	3.70	2.30	2.70	3.60	0.90	4.10	2.70	4.10	3.50	0.50
1990	2.93	3.08	1.95	3.60	2.25	2.75	3.60	0.90	3.97	2.60	4.03	3.28	0.50
1991	2.77	2.97	1.80	3.50	2.20	2.80	3.60	0.90	3.83	2.50	3.97	3.07	0.50
1992	2.60	2.85	1.65	3.40	2.15	2.85	3.60	0.90	3.70	2.40	3.90	2.85	0.50
1993	2.43	2.73	1.50	3.30	2.10	2.90	3.60	0.90	3.57	2.30	3.83	2.63	0.50
1994	2.27	2.62	1.35	3.20	2.05	2.95	3.60	0.90	3.43	2.20	3.77	2.42	0.50
1995	2.10	2.50	1.20	3.10	2.00	3.00	3.60	0.90	3.30	2.10	3.70	2.20	0.50
1996	2.10	2.50	1.20	3.10	2.00	3.00	3.60	0.90	3.30	2.10	3.70	2.20	0.50
1997	2.10	2.50	1.20	3.10	2.00	3.00	3.60	0.90	3.30	2.10	3.70	2.20	0.50
1998	2.10	2.50	1.20	3.10	2.00	3.00	3.60	0.90	3.30	2.10	3.70	2.20	0.50
1999	2.10	2.50	1.20	3.10	2.00	3.00	3.60	0.90	3.30	2.10	3.70	2.20	0.50

Table A3: Indices for union denisty

Year	*BE*	*DE*	*DK*	*ES*	*FI*	*FR*	*GR*	*IE*	*IT*	*LU*	*NL*	*PT*	*SW*	*UK*
1973	0.48	0.32	0.62	0.09	0.61	0.22	0.37	0.53	0.43	0.53	0.36	0.61	0.72	0.50
1974	0.49	0.34	0.65	0.09	0.63	0.22	0.37	0.54	0.46	0.53	0.36	0.61	0.73	0.52
1975	0.52	0.35	0.69	0.09	0.65	0.22	0.37	0.56	0.48	0.53	0.38	0.61	0.74	0.54
1976	0.53	0.35	0.73	0.09	0.68	0.21	0.37	0.57	0.50	0.53	0.37	0.61	0.75	0.55
1977	0.54	0.35	0.74	0.09	0.66	0.21	0.37	0.57	0.50	0.53	0.37	0.61	0.78	0.57
1978	0.53	0.35	0.78	0.09	0.67	0.21	0.37	0.58	0.50	0.53	0.37	0.61	0.79	0.57
1979	0.54	0.35	0.77	0.09	0.68	0.19	0.37	0.58	0.50	0.53	0.37	0.61	0.80	0.57
1980	0.53	0.35	0.79	0.09	0.69	0.19	0.37	0.57	0.50	0.53	0.35	0.61	0.80	0.56
1981	0.53	0.35	0.80	0.09	0.68	0.18	0.37	0.57	0.48	0.53	0.33	0.61	0.81	0.55
1982	0.52	0.35	0.80	0.10	0.68	0.17	0.37	0.56	0.47	0.53	0.32	0.61	0.82	0.54
1983	0.52	0.35	0.81	0.10	0.69	0.16	0.37	0.57	0.46	0.53	0.31	0.61	0.83	0.53
1984	0.52	0.34	0.79	0.12	0.69	0.15	0.37	0.57	0.45	0.53	0.29	0.61	0.84	0.53
1985	0.51	0.34	0.78	0.12	0.69	0.14	0.37	0.56	0.43	0.53	0.28	0.56	0.84	0.51
1986	0.49	0.34	0.77	0.12	0.70	0.13	0.35	0.53	0.41	0.53	0.27	0.51	0.84	0.50
1987	0.49	0.33	0.75	0.12	0.71	0.12	0.34	0.51	0.41	0.53	0.24	0.46	0.84	0.49
1988	0.48	0.33	0.74	0.13	0.72	0.12	0.33	0.52	0.41	0.52	0.24	0.41	0.82	0.47
1989	0.49	0.33	0.76	0.13	0.73	0.11	0.32	0.53	0.40	0.51	0.24	0.37	0.82	0.45
1990	0.50	0.32	0.75	0.14	0.73	0.10	0.31	0.52	0.40	0.49	0.24	0.32	0.80	0.44
1991	0.52	0.33	0.76	0.16	0.75	0.10	0.29	0.53	0.40	0.48	0.25	0.32	0.80	0.43
1992	0.53	0.32	0.76	0.18	0.77	0.10	0.28	0.51	0.40	0.47	0.25	0.32	0.83	0.41
1993	0.54	0.30	0.77	0.20	0.79	0.10	0.27	0.50	0.40	0.46	0.24	0.32	0.86	0.40
1994	0.54	0.29	0.77	0.20	0.79	0.10	0.26	0.48	0.39	0.45	0.25	0.32	0.91	0.38
1995	0.54	0.27	0.77	0.18	0.80	0.10	0.24	0.46	0.39	0.43	0.24	0.32	0.90	0.37
1996	0.56	0.28	0.79	0.18	0.80	0.10	0.26	0.46	0.38	0.44	0.25	0.32	0.88	0.35
1997	0.59	0.28	0.81	0.17	0.79	0.10	0.27	0.46	0.38	0.46	0.25	0.31	0.86	0.34
1998	0.61	0.29	0.82	0.17	0.79	0.09	0.28	0.45	0.37	0.47	0.26	0.31	0.84	0.33
1999	0.64	0.29	0.84	0.16	0.79	0.09	0.30	0.45	0.37	0.48	0.26	0.31	0.83	0.32

Table A4: Indices for bargaining coverage

Year	*BE*	*DE*	*DK*	*ES*	*FI*	*FR*	*GR*	*IE*	*IT*	*NL*	*PT*	*SW*	*UK*
1973	0.83	0.90	0.69	0.65	0.95	0.80	0.90	0.90	0.86	0.70	0.64	0.73	0.70
1974	0.84	0.90	0.70	0.66	0.95	0.81	0.90	0.90	0.86	0.71	0.65	0.74	0.71
1975	0.85	0.90	0.70	0.66	0.95	0.82	0.90	0.90	0.85	0.72	0.65	0.75	0.72
1976	0.86	0.90	0.70	0.66	0.95	0.82	0.90	0.90	0.85	0.73	0.66	0.76	0.72
1977	0.87	0.90	0.71	0.67	0.95	0.83	0.90	0.90	0.85	0.74	0.67	0.76	0.71
1978	0.88	0.91	0.71	0.67	0.95	0.84	0.90	0.90	0.85	0.74	0.68	0.77	0.71
1979	0.89	0.91	0.72	0.68	0.95	0.84	0.90	0.90	0.85	0.75	0.69	0.78	0.70
1980	0.90	0.91	0.72	0.68	0.95	0.85	0.90	0.90	0.85	0.76	0.70	0.79	0.70
1981	0.90	0.91	0.72	0.68	0.95	0.86	0.90	0.90	0.85	0.77	0.71	0.79	0.69
1982	0.90	0.91	0.73	0.69	0.95	0.86	0.90	0.90	0.85	0.78	0.72	0.80	0.68
1983	0.90	0.90	0.73	0.69	0.95	0.87	0.90	0.90	0.85	0.78	0.73	0.81	0.66
1984	0.90	0.90	0.74	0.70	0.95	0.88	0.90	0.90	0.85	0.79	0.74	0.82	0.65
1985	0.90	0.90	0.74	0.70	0.95	0.89	0.90	0.90	0.85	0.80	0.75	0.82	0.64
1986	0.90	0.90	0.73	0.71	0.95	0.89	0.90	0.90	0.85	0.81	0.75	0.83	0.62
1987	0.90	0.90	0.72	0.72	0.95	0.90	0.90	0.90	0.84	0.81	0.76	0.84	0.60
1988	0.90	0.90	0.71	0.74	0.95	0.91	0.90	0.90	0.84	0.82	0.77	0.85	0.58
1989	0.90	0.90	0.70	0.75	0.95	0.91	0.90	0.90	0.83	0.82	0.78	0.85	0.56
1990	0.90	0.90	0.69	0.76	0.95	0.92	0.90	0.90	0.83	0.83	0.79	0.86	0.54
1991	0.90	0.90	0.69	0.76	0.95	0.93	0.90	0.90	0.83	0.83	0.77	0.87	0.51
1992	0.90	0.91	0.69	0.77	0.95	0.94	0.90	0.90	0.82	0.84	0.75	0.88	0.47
1993	0.90	0.92	0.69	0.77	0.95	0.94	0.90	0.90	0.82	0.84	0.73	0.88	0.44
1994	0.90	0.92	0.69	0.78	0.95	0.95	0.90	0.90	0.82	0.85	0.71	0.89	0.40
1995	0.90	0.93	0.69	0.78	0.95	0.96	0.90	0.90	0.82	0.86	0.69	0.90	0.37
1996	0.90	0.93	0.69	0.79	0.95	0.96	0.90	0.90	0.81	0.86	0.67	0.90	0.33
1997	0.90	0.94	0.69	0.79	0.95	0.97	0.90	0.90	0.81	0.87	0.65	0.91	0.29
1998	0.90	0.94	0.69	0.80	0.95	0.98	0.90	0.90	0.81	0.87	0.63	0.92	0.26
1999	0.90	0.95	0.69	0.80	0.95	0.99	0.90	0.90	0.81	0.88	0.61	0.93	0.22

Table A5: Indices for incidence of temporary employment

Year	*BE*	*DE*	*DK*	*ES*	*FI*	*FR*	*GR*	*IE*	*IT*	*LU*	*NL*	*PT*	*SW*	*UK*
1973	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1974	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1975	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1976	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1977	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1978	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1979	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1980	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1981	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1982	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1983	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1984	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1985	6.90	10.00	12.30	15.60	16.50	4.70	21.10	7.30	4.70	4.70	7.50	14.40	13.00	6.90
1986	6.58	10.10	12.00	15.60	16.50	5.88	20.18	7.54	4.80	4.44	7.52	14.40	13.00	6.54
1987	6.26	10.20	11.70	15.60	16.50	7.06	19.26	7.78	4.90	4.18	7.54	15.40	13.00	6.18
1988	5.94	10.30	11.40	20.37	16.50	8.24	18.34	8.02	5.00	3.92	7.56	16.40	13.00	5.82
1989	5.62	10.40	11.10	25.13	16.50	9.42	17.42	8.26	5.10	3.66	7.58	17.40	13.00	5.46
1990	5.30	10.50	10.80	29.90	16.50	10.60	16.50	8.50	5.20	3.40	7.60	18.40	13.00	5.10
1991	5.30	10.48	11.06	30.92	16.50	10.92	15.24	8.84	5.60	3.28	8.24	16.74	13.00	5.46
1992	5.30	10.46	11.32	31.94	16.50	11.24	13.98	9.18	6.00	3.15	8.88	15.08	13.00	5.82
1993	5.30	10.44	11.58	32.96	16.50	11.56	12.72	9.52	6.40	3.03	9.52	13.42	13.00	6.18
1994	5.30	10.42	11.84	33.98	16.50	11.88	11.46	9.86	6.80	2.90	10.16	11.76	13.00	6.54
1995	5.30	10.40	12.10	35.00	16.50	12.20	10.20	10.20	7.20	2.98	10.80	10.10	13.00	6.90
1996	6.04	10.88	11.72	34.44	16.74	12.84	10.78	9.34	7.78	3.07	11.40	12.16	13.26	6.84
1997	6.78	11.36	11.34	33.88	16.98	13.48	11.36	8.48	8.36	3.15	12.00	14.22	13.52	6.78
1998	7.52	11.84	10.96	33.32	17.22	14.12	11.94	7.62	8.94	3.23	12.60	16.28	13.78	6.72
1999	8.26	12.32	10.58	32.76	17.46	14.76	12.52	6.76	9.52	3.32	13.20	18.34	14.04	6.66

B Results without bias adjustment

Table B1: Results from 5000 simulations on subperiods.

Sample properties:	1973–1979	1980–1989	1990–1994	1995–1999
No. of observations	1326	2180	1200	1108
No. of country-years	67	113	56	52
Average wage growth	14.99%	9.27%	6.68%	4.38%
Average inflation rate	10.68%	8.69%	4.99%	2.37%
Average unemployment rate	3.80%	8.71%	9.03%	9.21%
Observed wage cuts (Y)	5	55	42	104
Proportion of wage cuts (%)	0.38	2.52	3.50	9.39
Simulation results:				
Average simulated wage cuts	13	74	56	122
$\#(\widehat{Y} > Y^B)$	4765	4765	4679	4659
Probability of significance	0.047	0.047	0.064	0.068
Fraction of wage cuts prevented	0.615	0.257	0.250	0.148
Fraction of industries affected	0.006	0.009	0.012	0.016

Table B2: Results from 5000 simulations on regions.

Sample properties:	All regions	British Isles and Denmark	Core	South
No. of observations	5814	1565	2697	1473
No. of country-years	288	74	132	75
Observed wage cuts (Y)	206	53	122	29
Proportion of wage cuts (%)	3.54	3.39	4.52	1.97
Simulation results:				
Average simulated wage cuts	265	67	146	48
$\#(\widehat{Y} > Y^B)$	4995	4537	4811	4927
Probability of significance	0.001	0.093	0.038	0.015
Fraction of wage cuts prevented	0.223	0.209	0.164	0.396
Fraction of industries affected	0.010	0.009	0.009	0.013

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